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AUTHOR Sulzby, Elizabeth
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ABSTRACT

E. D. Hirsch's "Philosophy of Composition" (in press) reintroduces to communication theorists the "progress of language" thesis first developed, by Henry Bradley and Otto Jespersen around 1900, in the context of oral language. The thesis states that languages--English in particular--tend to become more simplified and flexible over time. This paper discusses recent investigations into the comprehension process as they relate to the progress-of-language thesis. While the investigations do not conclusively support the thesis, they do suggest parallels between historical and individual language development and between oral and written language. The paper describes aspects of comprehension common to both oral and written language, characteristics of the encoding of thought into speaking and writing, the appropriateness of extrapolating from studies about one form of language to another, and areas of research useful to the development of reading and writing in the young. A bibliography is attached. (RL)

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The "Progress of Language" thesis: Its relevance for the development of readers and writers

Elizabeth Sulzby

Charlottesville

University of Virginia

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I would like to express my thanks to E. D. Hirsch, Jr., for the use of his manuscript to The philosophy of composition which should soon be available in final form from the University of Chicago Press. Any distortion of Hirsch's ideas is unintentional; however, our pedagogical goals are somewhat different and I may have inadvertently used Hirsch's ideas to support some of my own. I may have been so strongly guided by schemata from my own head that I also missed many of Hirsch's provocative points.

I also owe thanks to numbers of fellow students of reading and writing. I am reminded of an eighteen-month-old daughter of a graduate student this year who convinced me that, in some form, she could read. She talked to the books and knew what books were for. My ninety-two year-old aunt recently called me to her bedside to "listen to this letter" from a close friend whom she called "a writer who creates images for you."

March 1977

The "Progress of Language" thesis:
Its relevance for the development of readers and writers

I see at least 3 major scrands of investigation into how a person learns to read and to write, or to comprehend and to produce communications. One line of investigation asks what is in the written text, or in the oral discourse (Frederiksen, in press; Grimes, 1975; Kintsch, 1974; Meyer, 1976 and in press). The second, closely complementary line of study questions the nature of the comprehender. What governs what the reader or listener will remember from text or from discourse in any given task for remembering (Anderson, 1976 and in press; Frederiksen, in press, Marshall, 1976; Meyer, 1976 and in press; Rumelhart, in press; Smiley et al., in press; Thorndyke, in press)?

The third line of exploration is one recently being brought back into focus by E. D. Hirsch, Jr., in The philosophy of composition (in press). Hirsch draws our attention afresh to the notion of the historical phenomenon called "the progress of language," a notion long out of vogue. This notion, however, bears re-examination in light of our re-discovery

of early scholars in psychology, pedagogy, and linguistics, as well as in light of contemporary speculations about the historical development of our use of the tools for communicating and comprehending.

In the past few years there has been a flurry of research in how we understand material that we hear and that we read. Much of this research addresses how what is in oral discourse or in written text affects the comprehender's recall of information. This research, which I will describe sketchily later, has vindicated ideas of E. B. Huey (1908) and F. C. Bartlett (1932).

Hirsch (in press) tackles the question of what the writer decides to encode and on what basis a composition teacher has any reason for suggesting ways to improve writing. Hirsch has, I believe, vindicated another set of ancient ideas--ideas about the history of the development of the English language best presented in the work of Henry Bradley (1904) and Otto Jespersen (1894, 1922, 1938). Bradley and Jespersen can be called the fathers of the "Progress of Language" thesis: that languages (and English in particular) tend to become more simplified, flexibly useful tools for the expression of human thought. I will restrict my comments to English, a language of a highly literate people with

the conservative impact of written language (Bradley, 1904; de Saussure, 1966/1915).

Under the Progress of Language thesis (picked up by Zipf, 1935; Martinet, 1972; Norman & Rumelhart, 1975), we can examine reasons why messages are easy or difficult to understand or to produce. Bradley and Jespersen held that oral messages in modern English are easier to understand psychologically than the same message would have been in earlier English; these oral messages are also easier to produce physiologically. Written messages in modern English can be easier to read. Hirsch, however, questions whether messages are easier to learn to encode in writing in modern English, even though he contends that language users (writers) have evolved conventions that enable writing to be more easily encoded for the intended comprehension to take place. We will look at current research in the use of these conventions later.

Hirsch believes that there is a difference between the oral and written forms of language, a difference greater and more important than the fact that one is emitted in sound and the second in graphs. He also believes that the Standard in any language has a special status psychologically. Whether we are speaking of the Standard in oral language or in

written language, we are not speaking of just another dialect among dialects, but of an importantly different dialect (Rogers, 1976; Smith, 1975). Hirsch adopts the term grapholect (which he credits to Einar Haugen) to refer to the Standard in both oral and written language. Furthermore, Hirsch uses the terms oral speech and written speech to indicate strongly that he is speaking from a performance, rather than from a competence standpoint. I will use the terms written language and oral language rather than add to the confusing proliferation of terms.

It is important to emphasize Hirsch's delineation between competence and performance (Chomsky, 1972) in the historical look at language and in the development of the young writer. Hirsch believes that the means of expression can be separated from the semantic intention of any composition (as do Anderson, 1976; Frederiksen, in press; Mandler & Johnson, 1977; Meyer, 1976 and in press; Richek, 1977, for examples). Paraphrase is possible; hence, compositions can be relatively more or less readable.

The separation of the means of expression from the semantic intention provides the basis for what Hirsch calls "relative readability" (as opposed to traditional, absolute readability). A college theme, for example,

can be considered both as the text, as the marks on the paper (its words, sentences, paragraphs, organization) plus the interaction with the reader. Historically writers have developed ways of making messages more comprehensible, so Hirsch looks at language diachronically. The young writer must learn conventions to help get across messages he wishes to encode; he too is developing diachronically. If the Progress of Language thesis is an accurate description of why language evolved as a better vehicle to express human thought, the young child may have advantages in learning these conventions. This speculation may form a framework for research and for pedagogy.

Hirsch writes primarily from the standpoint of a teacher of college composition courses; I will focus on writing and reading as they develop in the young child. First, I will describe the Progress of Language thesis in more detail; then I will relate it to some recent investigations into the comprehension process. I will address briefly some characteristics of deciding how and what to encode in speaking and in writing and will describe some aspects of comprehending oral language and written language by listeners and readers. I will discuss the appropriateness (or lack of appropriateness) of extrapolating from studies about one form of language to another and will suggest areas for research into the

development of the young reader and writer.

The Progress of Language thesis was developed in the context of oral language. In oral language people talk to one another; they communicate; they give one another feedback about the effectiveness of the message. In written language, however, the composer has to imagine an audience and the effectiveness of the communication upon that imaginary audience. Jespersen (1894), Bradley (1904), Zipf (1935), and Martinet (1972) all have claimed that language has changed in response to the needs of the speakers and hearers of the language and that language change is motivated (although not consciously motivated) by the discovery of easier means of communication.

Man is psychologically and physiologically lazy--or viewed more charitably, efficient. Both Jespersen and Bradley insisted that English and other languages tend to become more simplified tools for the expression of human thought. Jespersen, in particular, was writing in response to Schleicher's popular idea that languages were decaying, were becoming corrupted in modern times. Jespersen pointed out the historical fallacy of this reasoning but he also invoked a criterion

by which to measure progress. Jespersen's criterion for what Hirsch calls communicative efficiency was that the language require minimum effort on the part of the speaker to create maximum effect on the part of the hearer. Zipf (1935) called this criterion the "principle of least effort" (cf. de Saussure, 1966/1915, pp. 148-149). Zipf attempted to apply statistical analyses to document the reality of the principle. Using a metaphor from information processing theory, Martinet (1972) writes of cost: we as speakers present information selectively, expending as little effort as possible, to convey the most information possible within a given context (cf. Bates, 1976; Grice, 1975; Halliday, 1973).

Jespersen (1922) gave this outline of the ways in which languages (here he was analyzing Indo-European languages in general, but he and Bradley speak of these same changes in English) have progressed:

1. The forms are generally shorter, thus involving less muscular exertion and requiring less time for their enunciation.
2. There are not so many of them to burden the memory.
3. Their formation is much more regular.
4. Their syntactic use also presents fewer irregularities.

5. Their more analytic and abstract character . . . render possible a great many combinations and constructions which were formerly impossible.
6. The clumsy repetitions known under the name of concord have become superfluous. (p. 364)

Bradley's delightful little book (1904) illustrates each of these simplifications with examples of meanings that can be expressed by modern English far more delicately than by older English. For example, I can write, "Bradley does support Jespersen's argument," and convey a shade of meaning different from when I write, "Bradley supports Jespersen's argument." The analysis or breaking apart of auxiliaries from verbs is one way in which modern English becomes more useful to the speaker (or writer).

I mentioned that Hirsch began by examining language diachronically. In recent years, linguists have tended only to describe language historically, if they address the question of diachronic linguistics at all; they tend not to evaluate it. Martinet (1972) carefully points out that, in taking his stance on the issue of the Progress of Language, he has adopted a

minority position among linguists. However, I think that linguists opposed to the notion of the Progress of Language (for example, Labov, 1972) have had other purposes in mind, such as the need to examine variation in language (Fleming & Ohnmacht, 1976; Weinreich, Labov, & Herzog, 1968). Both Bradley (1904) and Jespersen (1894) were careful not to suggest that languages have become better able to express meaning with optimum levels of effort in any sort of straight-line, conscious movement toward a goal which language users had in mind.

Nevertheless, Jespersen (who traced the idea of least effort back to von Humboldt) claimed that modern English, like other Indo-European languages, has progressed. In modern English, as compared with its ancestors, we find shorter and fewer grammatical forms. Forms and their functions are more regular and are more abstract. They require less effort on the part of the hearer (or reader) to understand. These changes, Jespersen contended, have come about in a roundabout manner, not through deliberate choices of language users to make the system more communicatively efficient. (Good non-evaluative histories of language change are Moore & Marckwardt, rev. 1969, and Strang, 1970.)

Jespersen's argument was so persuasive that, in 1917, Sturtevant declared that his work had established almost beyond doubt the notion that language change has been in the direction of improvement. By the time of the 1960 reprint of Sturtevant's book, the notion was decidedly out of vogue. Sturtevant's editor, Eric P. Hamp, described the stance on progress as the weak point of Sturtevant's book, otherwise a classic in his opinion. M. E. Wrolstad protested this attitude toward the historical examination of language change in the Winter 1976 issue of Visible Language, characterizing himself as an iconoclast.

Evidence of the change in vogue toward the Progress of Language thesis can be found in the work of Bloomfield (1933) and Pyles (1971) who mention language changes without any attention to the notion of progress; other linguists like Labov (1972); Weinreich, Labov, & Herzog (1968); and Householder (1971) concentrate primarily on sound changes without mentioning progress, although they do address the question of ease. Hoenigswald (1966) actively attacks the idea of Progress of Language; he argues that the notion is circular, reflecting a variant of the "is-ought" fallacy--that of interpreting what did happen as if it were necessarily that which should have happened.

Hoenigswald claims, further, that there have been language changes of a major sort which dispute Jespersen's claims. He mentions languages which have moved from analytic to synthetic as well as those which have moved from synthetic to analytic. (Jespersen had used the movement from synthetic to analytic to refute Schleicher's decay theory.) It remains to be seen whether these changes affect the ability to convey meaning in a detrimental manner.

In response to Hoenigswald, Labov, and other modern linguists examining recent sound changes, it is appropriate to mention Jespersen's and Bradley's foresight about the slowing of change. They did not indicate that English, or any language of a modern literate society, would change at the rate that it had during the era in which literacy was rapidly developing. Hoenigswald's paper, for instance, is based primarily on sound and lexical changes. Jespersen and Bradley had predicted that, with standardization due to the more regular orthography involved in printing for a highly literate language community, change would be contained primarily in these areas, sound and lexicon.

The idea of progress in language depends upon both an abstraction (similar to the idea of a competence, Chomsky, 1972) and of a diachronic view of language (which may be opposed, in one sense, to a competence theory). The study of composition seems to require

an investigation of language performance, not an abstraction, nor a "competence." The kind of abstraction needed to make the claim for the Progress of Language should not be in conflict with a performance model for composition, yet linguists like Labov seem to reject the long abstract view and to prefer the short-term study of variability in language performance. In fact, Labov (1972) claimed:

> No linguist would today endorse [the] point of view about the survival of ["the better, the shorter, the easier forms"], which runs exactly counter to our notion of the arbitrariness of the linguistic sign. Languages do not seem to be getting better and better, and we see no evidence for progress in linguistic evolution. . . . (p. 27)

Fleming & Ohnmacht (1976) find Labov's position appealing because he suggests that we should study variability in language as an essential counterpart to invariance even though it makes investigations messy and aesthetically less pleasing. They are writing from the standpoint of research in reading development (as I am) in which we are constantly looking at variability along with invariance (of a sort).

Labov's objection seems to relate to a

misinterpretation of the notion of progress; he admits this (pp. 323-325) when he claims that the principle of least effort has been overemphasized and related to uniformity rather than to functional use of diversity. Working with oral dialects with a strong stress on communicative facility within a diversity of language changes, Labov does not adequately address the historical arguments of Jespersen or Bradley, in my opinion. He is, I think, justified in pointing to the inadequacy of Zipf's interpretation of his data; he does not, it seems to me, disprove the invariance which Zipf pointed out.

It is interesting to notice the speech act theorist John Searle (1971) assenting to Zipf's law in speaking of how much to promise:

> Parenthetically, I think this condition is an instance of the sort of phenomenon stated in Zipf's law. I think there is operating in our language, as in most forms of human behavior, a principle of least effort, in this case a principle of maximum illocutionary ends with minimum phonetic effort. . . . (p. 50)

Lest the reader lose uptake on Searle's exact referent here, he was discussing a condition governing promising in which it is necessary to promise only if it is not

obvious that one will do the act in the "normal course of events." The bridegroom who promises his bride that he will not desert her when they return from the honeymoon is uttering a promise that should be needless, yet by uttering that promise he probably has aroused her suspicion that something is going awry in their relationship. A theorist might argue that this bridegroom hasn't learned when to encode and how much of his thoughts to encode, a problem that we see in developing speakers and writers. He would not be using the principle of least effort. In fact, after his statement he might be required to use maximum effort to once again set things right with his bride.

The principle of least effort and the notion of Progress of Language both involve an interplay between impetus toward change and conservativeness. (This statement does not relate to the bride/bridegroom situation, but back to the main issue of language change.) Henry Bradley in particular was concerned with the psychological pressures toward progress. He foreshadowed modern studies in the functioning of memory in his discussions of the loss of inflectional endings; he described memory and forgetfulness working together alongside our seeming "instinct for consistency" (1904, p. 19), offset by the conservative

press of traditional usages.

Furthermore, Bradley claimed that we are forced to work to resolve ambiguities in the expression of meaning. He attacked the position, for example, that phonetic change would not destroy inflectional endings unless they were already useless. His view was that, when phonetic change makes forms the same, and hence ambiguous, speakers react; they use position (syntax) or they "regularize" one of the forms.

Overall, Bradley (1904, 1928) viewed language as a communicative tool, and man as a being with meanings to express by use of this tool. Similarly, Zipf (1935, 1940) marvelled at man's ability to concentrate on the expression of meaning yet, at the same time, to simplify his language forms. Strang (1970) thought Zipf's statistical surveys were oddly fruitless and that meanings could always be equally well expressed; yet she herself describes the "wear and tear" on our most frequent forms, the history of which she claims (cf. Moore & Marckwardt, 1969) is different from that of less frequent forms (Strang, p. 415).

Much of the analyses of Bradley, Jespersen, and Zipf (and Strang, for that matter) is addressed at the surface level of language, at the word level. Yet the word level seems to be inextricably tied to

the underlying meaning which is encoded (compare the work in artificial intelligence of Goldstein, Papert, & Minsky, 1976; Minsky, 1975; Winograd, 1975, 1976). Because of this tie to the surface level, Hirsch (in press) was thus able to replicate the kind of comparison that Jespersen (1984) used to substantiate his claim that the language overall has changed toward shorter and fewer forms to convey the same meaning. Hirsch took translations of the same passage from The Decameron from 1566, 1620, and the 18th century to make his comparisons. Jespersen had used syllable counts, but Hirsch was able to use not only comparisons of shorter lexical forms but also comparisons of conventions to help the reader comprehend. He mentions proleptic devices (such as my use of not only, but also in the preceding sentence) and the principle of joining the subject and verb more closely in print (and, thus, Hirsch thinks, in the mind of the reader).

It is interesting to note that the 1566 passage embeds long modifiers and clauses within the basic sentences, holding the reader in suspension over a long span of time/print. Deese (1976 and in progress) has found this characteristic in spontaneous speech. It is be likely that the writing of 1566 might be more like spontaneous oral speech. The idea that the passage

is more like spontaneous oral speech would fit with Bradley's and Jespersen's claims about the conservative nature of print, because in the 1620 and the 18th century passages Hirsch found the translators beginning to use devices that had evolved in written language to help the reader comprehend.

Besides illustrating the Progress of Language, Jespersen's and Hirsch's use of translations of the same passages across time helps build the case for the ability to paraphrase (Schank, 1973) or to express the same semantic intention in different ways. Yet the issue of paraphrasability does not seem to be resolved. Much of our research assumes that paraphrase is possible, yet we do not know what degrees of paraphrase there are and are only beginning to study when and how paraphrase makes a difference (Marshall, 1976; Richek, 1977; Singer, 1976; Walker, 1975).

Wrolstad (1976) writes about two "black boxes" in man's language history--that of language development historically and that of language development within the individual. Paraphrasability in translations across decades or centuries addresses the diachronic "black box" of the Progress of Language about which we are still speculating; the area of paraphrase with

the same semantic intention synchronically is addressed in the question of how individual man carves up his knowledge of the world (Winograd's declarative base) and uses arbitrary signs more or less skillfully (procedurally) to aid the reader in ascertaining the writer's semantic intention. Hirsch is a composition teacher, and his argument depends upon the validity of the claim that we may keep meaning the same while varying our ways of expressing it.

The analogy from artificial intelligence includes not just how we carve up our knowledge of the world (Schank, 1973), but how we use procedures to manipulate the base (Bransford & Franks, 1976; Minsky, 1975; Rumelhart & Ortony, in press; Winograd, 1975) and how we use pointers to aid in using the procedures and base. These pointers may be analogous to Hirsch's conventions. (Minsky speaks of pointers to substructures; cf. Rumelhart & Ortony on how explicit the system needs to be to call up subschemata for comprehension.)

Taking this analogy to oral language, we predict and operate on what is "there" in the speech flow; we couldn't understand the flow without the search procedures (our expectations), however. Reading seems somewhat different; but this difference may be specious. The bits and pieces <u>seem</u> to be carved up and waiting

to be understood with left-to-right, return sweep search procedures, with meanings compounded from the marks on the paper. This difference, as I indicated, is too simplistic; we understand that we do not look for meanings on the page alone (Anderson, 1976; Smith, 1975). We have expectations (Stauffer, 1969, 1970). Reading seems to be an interactive process, not just a receptive or even a strictly constructivist process; the reader interacts with what the writer has written and with how he has written it (Bransford & McCarrell, 1974; Rumelhart, 1976).

The ability to use varied means to convey the same semantic intention and the ability to use procedures and markers to facilitate reading leads us to consider the differences between oral and written language (and between reading and writing) which Hirsch finds crucial to a theory for teaching and evaluating composition. Bradley (1928) had claimed that written and oral language developed differently, although coextensively, and that now the printed convention is able to slow change in oral language.

Frank Smith (1975) suggests that over-simplifications about the relation between written and oral language have led to such statements as: "Writing is speech written down." Investigations

in language acquisition and the development of reading and writing (Tucker, 1976) indicate that we have indeed oversimplified the relationship(s) between written and oral language. For instance, I now speculate that much of the oral input from such sources as radio and television may be a hybrid between oral and written language. Announcers reading or ad-libbing from scripts, soap opera and comedy stars operating from cue cards, and stock reactions on talk shows may all add conventions from written code into the oral codes (compare Labov on code switching, 1972, and the work of Halliday on the functions of language, 1973b).

Similarly, Wrolstad in his manifesto for Visible Language (1976) claims that visible (written) and audible (oral) languages are discrete manifestations of language in general (comlang) each with direct channels of control and with interaction between the two manifestations. He suggests further that man has evolved layers of ways to convey meaning and that

> It is the layer upon layer of approximation that makes it all but impossible to overstate the complexity of language. (p. 11)

Wrolstad thus warns against our propensity to parallel

oral language with written language directly:

> [W]e can neither adequately explain nor fully understand one language system in terms of the other. (p. 13)

I find that I have a tendency to parallel the two systems simplistically; Carl Frederiksen (in press) has done this (with a slight disclaimer), as have other investigators in memory for text (Meyer, 1976). Frederiksen has taken an old tradition in reading comprehension research (cf. the informal reading inventory) and has assumed that memory for written material read orally and memory for reading that same material silently operate as one system. This common operation assumption may or may not be warranted; some investigators have begun to address this issue (Goldman, 1977; Smiley et al., in press; Tucker, in progress; Walker, 1975).

Hirsch's work addresses what the writer needs to encode to help the reader comprehend. After decrying the inadequacy of empirical studies in composition, Hirsch decided that it may not be time for such studies; perhaps the only defensible strategy at this stage is to theorize and make deductions from theory. There seems to be a growing body of research in comprehension for oral discourse and for written text that addresses the nature of the discourse or text as written by a

mature writer for a reader. This work does not yet, to my knowledge, address the nature of the developing young writer.

There is currently, in the areas of language research with which I am acquainted, a movement toward a study of performance (versus competence) with fairly strong theoretical assumptions about the nature of the process (Anderson, 1976; Rumelhart & Ortony, in press, for example). I would assume that Hirsch is correct and that basic research is an appropriate state for the study of the acquisition of writing. In my discussions of the possible differences between reading, writing, speaking, and listening, I will embellish my summaries of some existing research with my speculations about profitable areas of research into the development of the young writer, as well as describing the research assumptions about mature writers.

A way of generalizing about much research into the relationship between oral language and written language is to consider what is encoded, how much is encoded, and how ideas are encoded in messages.

Another way is to study how material in oral language and in written language is comprehended by listeners and by readers. These are the two complementary strands of investigation I mentioned at the beginning of the paper, along with the third strand of Progress of Language. The first two strands may be described as the development of schemata for production and for comprehension within individuals; the third strand may be considered as the development of schemata within a language group or culture over time.

I mentioned a movement toward the study of performance with assumptions about the process. I also mentioned that listening and reading are interactive processes, not just receptive or constructive processes. The assumptions behind these claims may be classified under such headings as schema, script, or frame theory (Anderson, 1975; Goffman, 1974; Minsky, 1975; Winograd, 1975). Since Bartlett (1932) used schema for want of a better term, I too will use schema. Schema theorists assume that we interpret the world (and oral and written material) from well-developed stereotypical frames of reference, or schemata, with only lower nodes open for filling (or being filled by default). In other words, we approach

experiences with highly developed, finely articulated expectations and we look for confirming signs; in reading passages, we look for signs or conventions that writers use to help us assume the schemata they have in mind for us. At least this is the assumption about mature humans.

But how are messages, oral or written, produced? Martinet (1972) stresses that we encode just as much as we need to; we attempt to lower the cost, or effort, required in communicating; we will, however, increase the cost if our communication is not effective and if we strongly desire to communicate.

Children become able fairly early to make decisions about what needs to be included in an oral message, according to the research of Elizabeth Bates (1976). Bates and her colleagues have traced the development of metapragmatics, or the conscious control of the pragmatic abilities, including the development of control over degrees of politeness. Bates had to use clever means of making children want to display this control, of course; the children had to ask politely for goodies. Then they had to ask even more politely, then even more. They showed intriguing abilities to lower their voices, ask for "just a little bit," and work pragmatically for a goal. We do not know a great deal yet about how

children gain control of the various conventions of oral language, and even if we did we might not be justified in extrapolating from oral language development to the control of conventions in written language. This is an area open to research.

Deese (1976 and in progress) has stressed that we do not know as much as many linguists and psychologists have assumed about how adults express thoughts in language. He has recorded graduate seminars in which people are attempting to express abstract ideas and has demonstrated that people assume often that they have encoded ideas when in fact they have not. Deese has moved from results in verbal learning tasks in the laboratory to observing spontaneously produced speech and how it is encoded and comprehended (cf. Anderson et al., 1976, regarding differential effects of laboratory tasks versus real life demands).

Some research in encoding to oral language by children also involves comprehension tasks (Mandler & Johnson, 1977, for example). The studies point to the fact that children, as well as adults, can encode differently for different purposes (Brown, 1975; Norman & Rumelhart, 1975; Paris & Lindauer, 1976; Searle, 1976), according

to task demands.

If the task is a desire for something, as it was with Bates' children, people will encode with the amount of specificity they think is needed and this may be contextually determined. These situations may not call for more specificity, yet they may be different messages:

"Bring me the book." (The one book we own)

"Bring me the book." (The one I was just using)

"Bring me the Book." (The Bible in some religious homes)

Other situations call for more specificity. With many constantly used books in a graduate student's office, "Bring me the book," may elicit, "Which . . . book do you mean, colleague?" or some other polite request for specificity (cf. Grice, 1975; Norman & Rumelhart, 1975).

The listener has a task. He must decide how much the listener knows, how much he can get the listener to accept or call to mind. If he is telling a story, he wants the listener to do more than give "the willing suspension of disbelief" that we used to talk about; he wants the listener to re-create a framework or schemata that fits his own. The speaker has to assume that the listener has the

potential for knowing what he is talking about if he signals his intentions (through such conventions as making his references definite (Chafe, 1972)).

Norman & Rumelhart (1975) treat all communication as a problem of reference, a problem in referring to concepts intended. How one refers carries meaning also, so in some sense paraphrase is possible and in another it is not. Bartlett (1932) had raised a similar issue in discussing the differences between oral language and written language:

> A story told to auditors is never quite the same as a story told for readers. Even when the matter is identical, the manner is different. (p. 245)

Smiley et al. (in press) and Walker (1975) have made attempts to deal with the problem of how people refer to concepts in oral language and in written language by manipulating the material used.

Most studies about how material is encoded in writing concern a mature writer communicating to readers (or, in experiments, to listeners). A well-defined structure is considered to be more easy for the writer's reader to comprehend (Meyer, 1976 and in press). Meyer also has found

that some kinds of organization are better designed to help readers comprehend and recall material (cf. Mandler & Johnson, 1977; Marshall, 1976; Singer, 1976).

Organization of the ideas encoded in writing is important. However, Anderson et al. (1976) claim that "A text is never fully explicit" (p. 6). The writer must decide how much explicitness is appropriate for the semantic intention he has and for the imagined audience of the message.

Winograd (1976)discusses the fallacy of assuming that these two simple questions are factual, textually explicit questions whose answers are encoded in the accompanying paragraph:

1. Who was opening the box?
2. What was in the box?

> Tommy had just been given a new set of blocks. He was opening the box when Jimmy came in. (p. 6)

A cursory examination of the Tommy paragraph might lead one to think that the writer had explicitly encoded signals to an appropriate schema for answering the two questions.

Explicitness in encoding in written language may be partially dependent upon the length of the passage, upon the amount of time/print that the writer

gives the reader to activate an appropriate schema (cf. Thorndyke, in press). Here the evidence seems somewhat contradictory.

Richek (1977) paraphrased different form of anaphoric reference with different kinds of sentence complexity. (In the Tommy paragraph, he is an anaphoric reference to Tommy.) Richek found that children develop the ability to understand anaphoric reference rather late. She was, however, working with sentences in isolation. Her work would lead to the assumption that material for very young children should be written explicitly:

> Tom saw Suzie and Tom kissed Suzie; rather than,
> Tom saw Suzie and he kissed her; or
> Tom saw Suzie and kissed her.

Again, I would caution against extrapolating from one context for language to another.

Richek's work, while limited because her sentences were out of context, may relate to Hirsch's plea that writers use proleptic devices and thematic tags to aid the reader. Walker (1975) made a comparison between comprehension for written material and for spontaneously produced speech. To get his material he had two students talking in an interview setting. Then he turned the recording into written language. Of interest here was the type of changes that Walker

made. His spontaneous speech samples were filled with anaphoric references which he turned into more explicit thematic tags (to use Hirsch's term). Walker's materials were longer (like Thorndyke's) and had a context more easily recognizable by the hearer/reader (an interview). Walker found that recalls from the written versions were quantitatively similar but qualitatively different. The recalls from the written material, with conventions signalling the appropriate schemata, were more precise, more hierarchically structured. (Cf. Meyer's claim that a structure will be used if it is present in written material; Walker's study adds a kink because his listeners were also watchers of a videotape and thus had contextual information from a real-life setting.)

Other investigators have manipulated the conventions or signposts from the writer. Anderson (1976) and Pichert & Anderson (1976) report that passages which are ambiguous will be interpreted by a reader according to the reader's background (cf. Battig, 1975). Readers were given schemata for some ambiguous passages ("Read this passage as if you were a burglar"; "Read this passage as if you were a real estate salesman"). Predictions were made about what would and what would not be remembered from the different perspectives and

from a control situation where no perspective was suggested. These predictions held even when the protocols were scored for alternate schemata, for simple sequence of ideas presented, and for recency/primacy effects, leaving the conclusion that what was remembered was a function of the perspective from which one read the passage.

Hirsch has argued that good writing puts in signposts or conventions to guide the reader. Pichert & Anderson argue that the text cannot be fully explicit:

> Mature readers are able to approach text with different purposes or perspectives that can override conventions a linguistic community ordinarily uses to structure a text. In other words, our hypothesis is that structure is <u>not an invariant property of text</u>, but rather that it depends upon perspective. (p. 6) [Emphasis mine]

Similarly, they argue that idea units may be affected by perspective and hence should not be simply thought of as invariant aspects of text. Hirsch had, however, also argued that text is "relatively readable," or depends upon an interaction between the text and the reader.

A profitable line of research might settle the seeming contradictions between the studies presented. I mentioned earlier that we do not know what degrees of paraphrase are possible and how paraphrase makes a difference. Similarly, we do not know much about the degree to which explicit signs in the text control reader schemata. We do have scattered pieces of evidence but it would be profitable, I think, to vary the degree to which passages contain what Hirsch calls proleptic devices and thematic tags and other such signposts to the intended schema.

The encoding of messages in written material that we have been discussing have been done by mature writers for experimental purposes. They can be interpreted as pointing toward pedagogical suggestions; Meyer (1976) for example contends that implications from her research should be extended to the field of teaching writing skills. Her suggestions do not have immediate applicability that I can detect for the developing young writer.

> Writers must provide a well-organized text with clearly identified schemata and realize that some schemata are more effective than others in processing information. (p. 6)

The problem with a statement like Meyer's about schemata when applied to young writers is the problem of having young children envision that imaginary audience that Hirsch mentioned.

One line of research that might discover how children develop the ability to imagine an audience for their writing might be to study children's early dictated stories. (Dictated stories avoid the physiological stress of having the child do the handwriting and spelling parts of the encoding.) I have tentatively identified some characteristics that seem to indicate an awareness of an unseen audience. Children begin to make comments, executive-like: "Read that back." "Let me hear how that sounds." "Take that out; I don't need to say that."

Another line of research may be to induce the idea of an audience. Acting may be a means of inducing the idea of an audience plus getting depth of processing of the semantic intention (cf. Craik & Lockhart, 1972; Craik & Tulving, 1975; Paris & Lindauer, 1976). I attempted to help a group of gifted fourth graders write a script for a well-known folktale. I found that it helped to act out the tale numbers of times <u>before</u> the

children wrote the script, then break out portions of the play to discuss how much the audience could see and how much the audience would have to be told. (We also were able to discuss the use of stereotypical characters to help the audience get our schemata; here perhaps the term script is more appropriate.)

Another consideration in research about young writers is whether or not writing is viewed as a communicative tool. The effects of real-life needs for writing should be studied and might be manipulated through observing children in such situations as writing to pen-pals. We are now entering (like Deese) into the messy world of how language functions for people and leaving the clean world of the verbal learning laboratory.

There are many more avenues which should be followed in studying the development of young writers but little research that I could find. There is an increasing number of studies recently in how written material and oral material is comprehended and recalled by readers and listeners and most of these investigations involve schema theory.

A strong theoretical stance is being taken by many theorists that reading is not just decoding to speech, at which point comprehension automatically takes place. A further, more tentative stance is that the same process may underlie both comprehension for text and comprehension for material heard. This assumption has been held in reading education circles for a number of years as witnessed by the oral comprehension sections and hearing capacity sections of informal reading inventories as well as other standard diagnostic tools which purport to compare a hearing capacity with a reading comprehension level.

Susan Goldman (1976) paralleled listening comprehension and reading comprehension tasks for good, moderate, and poor readers. Her primary purpose was to investigate the acquisition of the appropriate interpretation of sentences using tell and promise (a replication of a Carol Chomsky study). She randomized the sentences used and order of presentation, but her sentences were of the same form, whether they were read by the children or heard by them. She found that with third, fourth, and fifth graders Chomsky's stages were replicated in the listening task but were not so clearcut in the reading task. She suggests that acquisition may simply transfer

to the reading task, although she does not present evidence for this conclusion. The interesting part of her study involved her results with poor readers as compared with better readers. She noted that the poor readers could decode these sentences seemingly and with the "easy" tell sentences there was no interference; however, with promise sentences the poor readers had difficulty. She speculated that the decoding might not be automatic enough with the poor readers so that when the comprehension task was greater (with promise), the "decoding inefficiency takes its toll" (p. 135).

Goldman claimed that the low readers had problems that arose from comprehension of language in general. She did not address directly the issue of whether one system underlies both comprehension for oral language and for written language. Also, her material was not material designed to be heard orally, but was designed to be read. (A second experiment using blocked presentation further complicated any interpretation of her initial experiment.)

Smiley et al. (in press) did attempt to control for the type of material used in paralleling comprehension for oral language and for written language. They discussed the difficulties in extrapolating from

from one language form to another. They decided to use what I have called a hybrid; they used stories that they thought came from an oral tradition (cf. Mandler & Johnson, 1977). There could be many problems with this solution of the material problem. From doing some Biblical exegesis and some work with a philologist, I know there are difficulties in determining what is "oral tradition" material; however we may be straining at a gnat and making distinctions too fine to matter to children. At any rate, the step that Smiley et al. have taken is an important one because they can address the question of whether comprehension for material presented orally and material presented to be read depends upon one underlying system.

Smiley et al. also wanted to investigate Thomas Sticht's idea that listening and reading become comparable at the seventh grade level; he held that listening was superior for younger children. The Smiley group also wanted to compare good readers with poor readers to investigate differences that might relate either to reading ability as such or be common to both reading and listening comprehension. They also wanted to compare the poor readers' results with results

of average younger readers from a previous study.

The students were seventh graders at or above grade level and Title I children in sixth, seventh, and eighth grades who were reading below grade level. The materials were two Japanese folk-tales with a Dale-Chall readability score of approximately 5.0, with approximately the same length and same number of idea units. The idea units had been rated. Each student had both treatments, with stories and order of treatment counter-balanced.

The students wrote recalls which were scored for gist. Good readers were better comprehenders in terms of amount recalled, but more importantly they paid more attention to importance of ideas and differentiation of levels of importance of ideas in both the listening and the reading modes. These results tended to indicate that there was a common process underlying comprehension in both modes, but Smiley et al. were cautious in their claims.

The recalls of the poor readers were quite different. They recalled the highest level ideas (as Meyer's work would predict) and differentiated these alone. All other ideas (and few were recalled) were not hierarchically ordered. Smiley et al. could not find recalls similar to these in the third or fifth grade recalls from the previous

study so they went to first grade and used the same stories. The first graders listened to the stories with earphones; then they retold the stories on tape, but the taping was done while they were telling the story to another child. The first grade recalls were similar in amount and structure to the sixth, seventh, and eighth grade Title I students.

I could quibble with the readability level of the stories for Title I children, except that the results were qualitatively and quantitatively similar in the listening conditions. Smiley et al. interpret these results to indicate a comprehension deficit, not just a decoding problem. They were careful not to conclude that comprehension of written material and oral material are dependent upon the same processes but that listening to and reading of the same material, here a hybrid between oral and written language, "at least suggests similarity of underlying processes" (p. 13).

Marshall (1976) found differences in recalls of college students whom she called not-so-fluent readers (community college students) and Cornell students whom she called fluent readers. She suggested that the fluent readers were less affected by implicitly stated relations and by changes in

passage structure (cf. Mandler & Johnson, 1977). They seemed to activate an appropriate schema for comprehension which she postulated came from wide reading. Furthermore, Marshall found recall patterns in the not-so-fluent readers to be similar to those found in the Smiley study. The community college students recalled ideas high in the structure and all other material recalled tended not to be differentiated hierarchically.

Mandler & Johnson (1977) address the question of what there may be in simple stories that children are exposed to that may help children activate appropriate schemata. They also ask what changes children make in simple stories that may expose their own comprehending or memory system(s). They, like the Smiley group, were sensitive to the type of material in relation to task demands. They, like Hirsch, hypothesized "a relation between . . . . the oral tradition and cognitive structures for remembering" (p. 114). Mandler & Johnson chose stories in an oral tradition, with appropriate cautions about the tentativeness of our knowledge about oral traditions. They considered the types of stories that have survived in oral traditions such as folktales, fables, myths, and asked what it was about these story forms that made them memorable.

The work of Mandler & Johnson, in particular, allows us to reconsider the issue of Progress of Language, but from a different perspective. They considered characteristics of stories that survived in oral traditions and intimated that memory constraints would affect the structures of such stories over time and that certain structures would require less effort to remember (and re-tell). They suggest, for example, the probability that Bartlett's "War of the Ghosts" story did not come strongly from an oral tradition because it does not have the strong story schemata that would have allowed it to be carried in memory by a culture through time--unless that culture had other distinctions for memory of stories which we do not yet understand.

After considering some characteristics of stories that have survived from an oral tradition (such as having the protagonist introduced explicitly and having an emphatic or even moralistic ending), Mandler & Johnson chose four stories for children to recall and predicted the types of recalls that the children would product. More importantly, they questioned whether schemata used to encode (and here they mean receive or comprehend, whereas I have been using the term to mean speak or write) are the same as those used to retrieve or recall.

Schemata seem to work three ways in comprehension: 1) they direct attention selectively; 2) they help keep track of what has already happened, and 3) they allow you to know if expectations have been fulfilled (can be stored) or are still hanging. Mandler & Johnson (1977) seem to be working on many of the same assumptions that Russell Stauffer (1969, 1970) used in discussing the basis for the directed reading-thinking activity (DRTA) which many reading teachers use with children. Stauffer had also described a directed listening-thinking activity (DLTA), utilizing the same steps of prediction or activation of old information (directing attention selectively); reading under the guidance of those predictions or of that knowledge base; and revising predictions and knowledge as more and more information is processed so that one decides whether or not expectations have been fulfilled and information can be stored.

Mandler & Johnson were concerned with the investigation, at this stage, of a grammar for simple stories based on Rumelhart's (1976) computer formalization (based on schema theory). Their work, I feel, leads in the direction of what Goldstein, Papert & Minsky (1976) call "computational epistemology" (p. 45). If this parsing system were put to work in a computer,

the computer would have to have metacomprehension: it would have to know what it knew, what it didn't know, and how it might come to know (Brown, 1975; cf. Stauffer again). In thinking about Winograd's block world (1975, 1976), the computer had to "know" what it knew, what it didn't know, and how it might come to know new information and integrate the new information into the old information. Goldstein, Papert & Minsky's work may suggest that as machines get smarter, we may discover that children (and adults) are smarter also.

To investigate such esoteric topics as epistemology, metacomprehension (and others such as pragmatic efficiency, degrees of knowing and trusting, strength of decision) which may affect how children and adults come to comprehend and to understand, we will probably have to start with more simple cases. Mandler & Johnson, in fact, did just that; they asked how children would comprehend and recall well-formed stories.

Mandler & Johnson began with the assumption that well-formedness in stories is not a well-defined category, so they decided to take stories that they thought were well-formed by having survived in an oral tradition and to test "what people can and cannot

remember and . . . the nature of the distortions that occur in memory" (p. 130). A crucial difference between their work and that of Frederiksen (in press) is the stance on when inferences take place. Mandler & Johnson assume that changes take place in the story schemata over time after the story has been heard; Frederiksen claimed that his work has "establish[ed] that inferences are generated during input processing" (p. 13). Mandler & Johnson want to distinguish between encoding (receiving) and retrieval (memory). A question that may be more one of definition than one able to be researched is "When does comprehension take place--when the sound is being emitted or the graphs examined? Or is comprehension that process that takes place over time? (Has the memory metaphor limited our understandings of learning? (Bransford & Franks, 1976))

Mandler & Johnson varied the time between when the stories were heard and when the recalls were taken with first graders, fourth graders, and adults. One recall was taken after a 10 minute interference task and the second after 24 hours. Realizing that task demands may also affect children's ability or willingness to utilize strategies for comprehension and recall (Brown, 1975; Paris & Lindauer, 1976),

they asked the subjects to retell the story to a familiar adult who was not present when the story was told (cf. Smiley et al., in press, with their first graders). The greatest difference under these conditions was the amount recalled as subjects get older. They claimed that children do use schemata to order their recalls, but that younger children exhibit some differences in how they utilize schemata. For example, young children will report outcomes and ignore attempts or reactions. Mandler & Johnson attribute this to the schemata which children use rather than to a lack of comprehension. With the action orientation that we comment upon about young children, these results seem unusual and may relate to depth of processing or task demands (cf. Paris & Lindauer, 1976). Frederiksen (in press) reported on an analysis of recalls in which pictures were used with a running narration in the active voice, indicative or progressive moods: "This is Michelle. . . ." The inferences that were second most frequently made with this task were event inferences.

Mandler & Johnson also suggested that poorly structured material may affect the recalls of young children more than those of adults:

> To the extent that young children have less processing capacity, presenting them with a poorly structured story may draw proportionately more of their processing resources away from succeeding propositions as they try to account for violations or lack of structure.
>
> We were also struck by the lack of structure in many of the simple stories used in school systems to assess comprehension. (p. 142)

The distortions that were made were those that attempted to bring the stories more in line with what Mandler & Johnson called ideal story schemata. They also cautioned, however, that some of the role of schemata may be due to developmental cognitive changes which Piaget pointed to as well as for development of specific schemata for stories. In other words, some reorganization in memory may be due to general cognitive development and other reorganization may be due to specific cognitive task strategies.

My review of the literature on child language acquisition (1976) has led me to believe that children de- and re-contextualize, to borrow terms from Bransford & Franks (1976) and Bransford & McCarrell (1974).

The early speech of children is contextual, tied to referents in the real world, as well as it is intentional or pragmatic (cf. Bates, 1976, on the evolution of the child's awareness of the need for encoding the topic versus simply making a comment). Children have to learn to refer to past events, to objects not present, to objects or ideas not able to be seen or touched. Then they have to decontextualize further in order to communicate with a nonpresent audience in writing; in doing this step, children have to learn to recontextualize for the reader. They have to create a context in the mind of the reader.

I have worked with children who begin to learn to read by using their own dictated stories, as I mentioned earlier. These children often tie "sentences" together with "and then" after "and then" after "and then." These awkward coordinates may be the beginnings of cohesive devices that could be further analyzed for their intended function in the child's writing (for example, "and then" could be intended to convey a causal rather than a temporal relationship for some children). Children often forget to introduce their protagonist in writing although they will insert a protagonist introduction into their recalls for

poorly structured stories (Mandler & Johnson, 1977). They will end stories abruptly ("THE END") when they tire of writing (or from some other motive), but they will intrude an emphatic ending in a story retelling.

These differences between story retellings and story or event writing may point to fruitful areas of investigation into the development of the young writer (and into the development of the young language user, generally). These differences seem to be related to what I call gaps in acquisition of a function for a particular use. (The word gap may not be appropriate, but it is the best word I can find now.) It may be, and this is speculation on my part, that there is a gap between use of a linguistic function in oral language; between conscious control of the function in oral language; between use of a function in written language; and between conscious control of the function in written language. Furthermore, the function is probably complicated by the use of different conventions in written language.

I find my thoughts drawn back again to the question of tasks relevant to children as young language acquirers, whether the acquisition be of oral language or of written language. Hirsch spoke of the Progress of Language in terms of "pieces" of language; Mandler & Johnson (1977, as well as Smiley et al., in press)

investigated story forms that could survive in the oral tradition over decades or centuries and the characteristics of this type of material. Research worth doing, by my definition, is that research which matches up with what we see children doing or trying to do. In my own schooling, I remember the times and times that my first grade teacher allowed us to act out simple stories, building bridges of desks for "Three Billy Goats Gruff," building houses of books for the third little pig, and so forth. Then when I began teaching I noticed a lack of time and emphasis in the curriculum for such "frills." I began to reincorporate drama for fairy tales, fables, and other folktales into my own teaching when I failed miserably to teach the bits and pieces of English to second language-speaking children. They learned in an active, story framework. Similarly, I found this approach to be successful in the kindergarten and first grades, in the classes for the deaf and hearing-impaired, in classes for slow learners. Then I found that I had to use it with the group of gifted children whom I mentioned earlier. I then learned that using a similar technique as a priming before reading a basal reader story led to better comprehension, more interaction with the story and with other students, and more fun for

any group of students.

I found the children learning more deeply by this simple step, the incorporation of dramatizations for stories. I then read research such as that of Paris & Lindauer (1976) in which young children were found to comprehend and infer unstated relations (the filler of the instrument role) equally well with older children when they acted out a sentence's meaning. These children were not able to call up strategies for remembering voluntarily (cf. Brown, 1975) but they could use the strategies.

Children are using a tool that Hirsch has suggested has evolved as a better means of expressing human meanings. It may be that if we give them humanly meaningful tasks in which to use that tool, children will exhibit more facility than we have thought possible. My observations are just that, observations; they must now be translated into action research with children in the messy world of pedagogy.

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